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THE AEROLOGICAL CONGRESS AT MONACO

THE international study of aerology, as the exploration of the atmosphere is now called, was begun by a small commission, including the writer as the American member, which was appointed at an International Meteorological Congress held at Paris in 1896. Although the commission bears the title "International Commission for Scientific Aeronautics," aeronautics serves only as the means of obtaining meteorological data in the free air. The work of the organization rapidly extended and five meetings were held in European cities before the session this year, which, by invitation of its honorary member, Albert I., Prince of Monaco, occurred during the first week of April in the new Oceanographic Museum at Monaco. The interest and importance of this, the sixth, reunion, served to bring together about thirty colleagues, from fourteen nations, the writer representing the United States Weather Bureau besides his own observatory at Blue Hill, where the first aerological observations in America were undertaken.

The meeting on April 1 was opened by Professor Hergesell, of Strassburg, president of the commission, who reviewed the progress made in exploring the air since the meeting at Milan three years ago, dwelling particularly on the extensive cooperation in the simultaneous series during six days in July, 1908, when balloons and kites were sent up from forty-four stations on land and sea in the northern and southern hemispheres. He emphasized the importance of determining the direction of the wind at different heights

by observing pilot-balloons with theodolites.

The Prince of Monaco welcomed his guests to the new Oceanographic Museum, which he hoped would become a repository for the results of soundings of the air as well as of the sea, and to his yacht, *Princesse-Alice,* on which were made marine and atmospheric soundings from the equator to beyond the Arctic Circle. In speaking of the last the prince described the launch and recovery of a *ballon-sonde* at sea.

At the business meeting which followed, Professor Hildebrandsson was named vice-president of the conference. The death of Professor Pernter was announced and Professor Trabert, his successor as director of the Austrian Meteorological Office, was chosen a member of the commission. Other new members elected were: M. Vincent, head of the Belgian Meteorological Service; Dr. Kleinschmidt, in charge of the Lake Constance Kite-station; Captain Ryder, director of the Danish Meteorological Institute; Professor Bjerknes, of Christiania; Dr. Bamler, of Munich; the directors of the Russian observatories at Irkutsk, Tiflis and Ekaterinburg, and these military aeronauts: Captains Voyer and Bouttieaux, of France: Le Clément de St.-Marcq, of Belgium, and Colonel Capper, of Great Britain.

The program for the conference was intended to include: (1) technical questions relating to balloons and instruments; (2) methods, organization and equipment of expeditions; (3) reports of researches during the preceding year; (4) other questions, including new projects.

The International Commission on the System of World-stations, a subcommittee appointed by the International Meteorological Committee, also met in conjunction with the Aeronautical Commission.

In the first group, Dr. Assmann, director of the Prussian Aeronautical Observatory at Lindenberg, described a new process of making *ballons-sondes* by dipping in a rubber solution instead of cementing sheets of rubber. The particles of dust in the latter cause the pores to open, but the dipped balloons expand five or six diameters without losing gas or bursting, permitting a greater height to be reached or a smaller balloon to be used. The parachute cords by friction on the rubber weaken it, or if two balloons are attached tandem, the smaller sets up oscillations injurious to the record; therefore Dr. Assmann proposes to put one balloon inside the other, one of them bursting on reaching the maximum altitude. He now employs a rubber captive-balloon of 14 to 17 cubic meters capacity, which loses no gas and therefore does not present pockets to the wind, and when carrying wire of 0.6 mm. diameter can reach an altitude of 4,000 meters. To avoid the chemical effect of insolation on the rubber the upper portion of the net is covered with a yellow fabric. A discussion followed as to the advantages of rubber and gold-beaters' skin for captive-balloons.

Dr. Assmann in another paper described an instrumental method to verify if, in the isothermal layer, the ventilation of thermometers is sufficient. While the upper inversion was shown as early as 1893 by the observations of Hermite and Besançon, yet for a long time it was attributed to insolation. This disturbance was later avoided by night ascensions and the descent of the balloon accelerated through letting gas escape by clockwork. The rubber balloon gives a very nearly constant velocity of ascent and this ventilation and a bright metal casing around the thermometer are generally thought sufficient. While it is doubtful whether extremely rare air can carry off heat from the thermometric strip, if there is no change when an artificial

current reinforces the downward draught, then the instrumental error may be assumed to be *nil*. Compressed air, contained in a globe of polished metal, is liberated against the thermometric strip during ten minutes by means of an aneroid barometer at the height when the inversion of temperature usually begins in cyclonic or anti-cyclonic conditions. This device has not yet been tried in the high atmosphere. The second part of Dr. Assmann's paper described an apparatus for ventilating thermometers in captive-balloons, especially during calm weather. A horizontal fan is driven by an electric battery, and a dipping-vane changes its direction of rotation when the descent begins.

In discussing the first subject, the president asked to have it noted that in spite of incredulity in some quarters regarding the reality of the upper temperature inversion, no member of this commission doubted its existence.

Professor Hergesell exhibited a new meteorograph for manned and captive-balloons having a bimetallic blade for temperature and a Vidi vacuum box for pressure, with an automatic ventilator for the former. This instrument made by Bosch, of Strassburg, weighs 1,200 grams and costs $75. A discussion followed as to the use of a straight bimetallic blade instead of a curved one.

Professor Palazzo, of Rome, described two devices for launching *ballons-sondes* at sea, in which the Bourdon barometric tube liberates one of the tandem balloons or lets out the gas. In discussing the necessity of limiting the time of the ascensions other devices for the same purpose were mentioned.

Professor Rotch urged the adoption of a uniform method of publishing the kite data and suggested the form used at Blue Hill and Mount Weather. He pointed out the confusion regarding the positive and negative sign attached to the temperature gradient in the official publication of the data derived from balloons and kites. Following the report of a committee appointed to consider the matter, the conference decided to express the gradient as positive when the temperature decreased with altitude and, in the published kite-observations, to give the simultaneous observations at the ground so far as possible.

Professor Köppen, of Hamburg, sent a memoir in which he proposed that, instead of the usual readings of the barometer, the data be expressed in absolute units of the C.G.S. system, and that the pressure be reduced to a height of 100 meters instead of to sea-level. This proposition was referred to the International Meteorological Committee.

Professor Bjerknes, of Christiania, read a paper on the theoretical application of upper air observations, in which he insisted that the object of aerological observations is to secure diagnoses about the momentary dynamic condition of the atmosphere. The series of ascensions should give data separated by such intervals as will permit the changes occurring between different diagnoses to be followed, so that by combining them it becomes possible to formulate laws by which the future state of the atmosphere may be predicted. He proposed to establish three diagnoses, namely, in the morning, at noon and in the evening, or at eight, one and seven o'clock, Greenwich time, but as the instrumental equipment will not permit all the diagnoses to be complete, that at noon is declared the principal one. At this moment kites and balloons with instruments are to be sent up and observations made at the ground which will enable the atmospheric conditions at different heights to be traced on charts. For the morning and evening diagnoses,

besides the ground observations, only pilot-balloons need be sent up, but sufficient in number to serve as a basis for charts of air movements.

Professor Bjerknes endorsed Professor Köppen's proposition to use dynamical units to express atmospheric pressure and wind-velocity, and in replying to questions as to the advantage of absolute measures he said that they avoided the introduction of constants in the application of dynamic equations and thus gave precision and simplicity. Tables facilitate the transformation of the old into the new units. Mr. Cave, of England, said that the upper-air observations in England were now published in the *Weekly Weather Report* in absolute units.

Mr. Cave read a request from his colleague, Mr. Dines, to have the *ballons-sondes* sent up about an hour before sunset to avoid insolation and yet allow them to be watched with a theodolite. Professor Rotch said that this practise had given good results in America, but the conference was unwilling to change the existing hour. It was later voted that beginning in July, complete observations be obtained as nearly as possible at 7 A.M., Greenwich time, as heretofore, but that pilot-balloons be sent up three times a day as desired by Professor Bjerknes during one of the monthly series of ascensions. There was also voted in this connection, a proposition of M. Vincent, to observe the state of the sky on the international days.

M. Teisserenc de Bort, of Paris, spoke on the result of his triangulation of *ballons-sondes* from 1898 to 1909 at Trappes and their importance for the verification of heights calculated by the barometer. The base-line used was 1,300 meters, but there is now a base of 5,000 meters. While the two methods agreed closely, the effect of hysteresis in the barometer was evident, though the temperature-correction was small. M. Teisserenc de Bort exhibited charts of some trajectories of his balloons, which showed that the cyclonic rotation ceases at a certain height and is replaced by a calm zone that marks the top of the adiabatic temperature-gradient. Above this the currents are from the south and west and opposed to the surface-wind. In the tropics, superposed currents occur with a sudden change in direction at about 7,000 meters, the absence of cyclonic storms accounting for the stratification. As to the changes of wind in the "stratosphere," or region of the upper temperature-inversion, M. Teisserenc de Bort had found only slight changes, but Messrs. Cave and Hergesell agreed that the velocity of the wind decreased there. Professor Hildebrandsson's observations of clouds confirmed those of M. Teisserenc de Bort with balloons, in showing a motion towards the north above 3,000 meters over a low-pressure area.

Continuing the technical questions, Professor Hergesell discussed the upward velocity of rubber balloons as a function of their lift and the use of pilot-balloons to determine the vertical currents of the atmosphere. In a closed space the velocity remains constant, so that, knowing the surface and free-lift of a balloon, its rate of ascent may be calculated, and there is little difference—about 4 per cent.—between the height obtained from angular observations of the balloon at one station, combined with its assumed velocity, and measurements of its height from the ends of a base-line. Successive observations of the displacement of the balloon enable the speed of the horizontal wind to be determined accurately. Often the vertical currents may be measured from angular observations at the corners of a triangular base, and a downward movement of 1.5 meters per second has been measured. In the discussion Captain

Hildebrandt, of Berlin, said that on the Peak of Teneriffe, pilot-balloons were driven down for three minutes at the rate of 2.5 meters per second and Gen. Kowanko, of St. Petersburg, mentioned the fact that when above a sea of clouds aeronauts found currents rising over the cloud summits and descending in the spaces between the clouds.

M. Teisserenc de Bort discussed the data relating to the upper isothermal stratum. Up to about 10 kilometers the decrease of temperature is almost adiabatic, then in the next 5 kilometers there is usually a rise in temperature of 8° to 10° C., with isothermal conditions up to at least 26 kilometers. The lower zone he calls the "troposphere," and the upper one the "stratosphere." The former is a region of violent atmospheric disturbances, for Hildebrandsson has shown that cyclones do not extend above the cirrus clouds, though anti-cyclones persist to greater heights, and therefore the stratosphere is lowest in the cyclone and highest in the anti-cyclone and its level sinks from the equator to the poles. The stratosphere is a region of interlaced currents and small vertical movements.

The following papers described the results of recent expeditions and investigations. General Rykatchef, director of the Central Physical Observatory, at St. Petersburg, exhibited charts of aerial soundings at various Russian stations. The higher level of the great temperature-inversion in anti-cyclones than in cyclones at St. Petersburg, and the isothermal condition prevailing in winter for several thousand meters above the ground at Asiatic stations, were some of the phenomena illustrated.

The most interesting report was presented by Professor Berson, assistant at Lindenberg Observatory, on his recent expedition to tropical East Africa and Lake Victoria Nyanza. On the coast and from a specially chartered steamer on the lake, *ballons-sondes,* pilot-balloons and kites were sent up. The observations over the equator, in the center of the continent, showed very low temperatures at great heights, as did the expedition of Teisserenc de Bort and Rotch on the equatorial Atlantic, but with the difference that over the African continent there was a trace of the isothermal layer. The vertical changes were as follows: adiabatic decrease of temperature to 13,000 meters, between 13,000 and 15,000 meters a small inversion, and above 17,000 meters isothermal conditions. Above the southeast monsoon the wind was south-southwest and three times a westerly wind was observed between 15,000 and 18,000 meters, above the great equatorial current from the east which is supposed to prevail at all heights.

Professor Palazzo, director of the Italian Meteorological Office, described his aerological expedition to Zanzibar and to the east coast of Africa, in which *ballons-sondes* and pilot-balloons were launched from an Italian cruiser. Professor Hergesell gave some results of balloons sent up from a German cruiser in the neighborhood of the Canary Islands, and on the Peak of Teneriffe. He showed the effect of the distribution of barometric pressure on the trade-wind, which is especially influenced by the displacement of the center of maximum pressure over the North Atlantic. Professor Hergesell reaffirmed his belief that the effect of the Peak on the wind extends up to 6 kilometers above sea-level. Professor Rotch presented his volume giving an account and a discussion of the first observations with *ballons-sondes* in America, which were made at St. Louis from 1904 to 1907.

The communications relating to new projects included the promises of Dr. van

Bemmelen, of Batavia, to establish a kite station there, and of Mr. Davis, a director of the Argentine Meteorological Office, to do the same in this country. M. de Massany gave an account of an aerological station about to be established at Kecskemét on the plains of Hungary, which plan received the approval of the commission.

Professor Hergesell spoke of the new observatory on the flank of the Peak of Teneriffe at a height of 2,400 meters, which he had just inaugurated in portable buildings furnished by the German emperor. Aerological stations in Spitzbergen and Teneriffe are of particular value and the desirability of the latter was expressed at the Milan Conference. Col. Vives y Vich, the Spanish military representative, took exception to some of Professor Hergesell's statements, and the following facts were agreed upon: The Aeronautical Commission was ready to establish an observatory on the peak with the aid of the Prussian government. The Spanish government objected, but recognizing the scientific value of the enterprise, it provisionally accepts the use of the temporary buildings offered by the Prussian government, through Professor Hergesell, until permanent buildings can be erected. Until such time the buildings will be considered Spanish property, and while the observatory will be open to savants of all countries, no preference can be given to Germans. The conference expressed its thanks to both the German and Spanish governments, and especially to the Spanish military aeronauts, for creating this observatory.

Professor Assmann read a suggestive paper on the application of aerological observations in aerial navigation. He cited the observations which were being made in the free air at various observatories and by expeditions on the oceans. Eventually it will be possible to construct synoptic charts of the upper air which will enable predictions to be made of great value for aerial navigation. Even now, before a dirigible balloon ascends from Berlin, the observations in the free air from four stations are consulted. These views were approved by several members and Dr. Bamler thought it possible to obtain continuous registration in a captive-balloon maintained at a constant height. In this connection the attention of the aero clubs was called to the importance of making meteorological observations in all manned balloon ascensions and the assistance rendered in this respect by the Vienna Aero Club and the Austrian Minister of War was acknowledged.

It was voted to exchange copies of the traces of the automatic records between members on their request, and to send titles of new aerological publications to the *Fortschritte der Physik* in Germany and to the *Monthly Weather Review* in the United States. The next meeting of the Commission will be held at Vienna in the autumn of 1912.

Three sessions of the Commission on the System of World-stations were held with M. Teisserenc de Bort as president and Professor Hildebrandsson as secretary. The former made a report on his project for telegraphic meteorological stations and the latter explained his proposition to the International Committee in 1899 to establish meteorological stations around the great centers of action on the globe and showed the compensating types of weather occurring simultaneously in different regions. Thirty-eight stations, at important points around the globe between 70° N. latitude and 50° S. latitude, were selected, at which, besides the ordinary elements, the direction of the upper clouds, the temperature of the sea and the insolation at fixed altitudes of the sun are to be observed at the hour of the usual morning observation, except the

insolation, which is for the preceding day. For the study of the centers of action, the monthly means of observations are to be sent by the cooperating institutes to the president of the commission, but for the other system of stations the observations are to be telegraphed every day, or, if this is impossible, the weekly means can be telegraphed and, like the daily observations, published in the weather bulletins of the respective countries where they will be available for study.

Although the week was chiefly occupied with the scientific sessions, the prince entertained members of the commissions several times at the palace and on his yacht, the *Princesse-Alice,* where he himself participated in some oceanographical investigations. These, as well as the aerological work of the prince, were illustrated by an evening lecture given by his aide-de-camp, M. Bourée. The little time remaining was agreeably filled by a visit in automobiles to the Nice Observatory and by a performance at the opera of Monte Carlo. To the writer the prince expressed the desire that with the completion of the Oceanographic Museum, the principality of Monaco should be not only a pleasure resort, but also become a scientific center, and the Aerological Congress prove the precursor of meetings of a similar nature there.

A. LAWRENCE ROTCH
BLUE HILL METEOROLOGICAL OBSERVATORY,
HYDE PARK, MASS.

THE DARWIN CENTENARY

ADDRESS IN REPLY TO THE RECEPTION OF DELEGATES[1]

CROSSING the Atlantic in honor of Darwin and rejoicing in the privilege of uniting in this celebration of his birth, we desire, first of all, to render our tribute to the University of Cambridge.

To no other institution in any country may we turn with such a sense of filial gratitude. Through John Harvard, of Emmanuel, Cambridge became the mother of our colleges. Did not Emmanuel beget Harvard, and Harvard beget Yale, and Yale beget Princeton and other descendants to the third and fourth generation? We thus salute to-day the venerable but ever-youthful ancestor of many of the American universities, academies and institutes of science, national and state museums, represented here, and in large part guided by true sons of the true daughters of the alma mater on the Cam. Through the survival of the best, our political guidance is also passing more and more into the hands of men trained in these same daughter colleges. A son of Yale succeeds a son of Harvard as president of the United States. If your university men are leading the empire in times of stress, ours are leading the nation through the more perilous, because more insiduous, times of prosperity. Thus in ever-widening growth is the influence of the Cambridge heritage. "Sir Walter," remarked Queen Elizabeth, "I hear that you have erected a Puritan foundation." "No, madam," he replied, "far be it from me to countenance anything contrary to your established laws; but I have set an acorn, which, when it becomes an oak, God alone knows what will be the fruit thereof."

The other offspring of Emmanuel, of Trinity, of Christ's and of the many pious foundations of the old university, are the great men, too numerous to name, but among whom there especially rise in our minds Newton, Clerk-Maxwell, Balfour, and above all, Darwin. Newton opened to us the new heavens, and Darwin the new earth. Clerk-Maxwell, with Herz, enabled us to converse with you through the blue ether. The well-beloved Balfour revived the spirit of Von Baer; would that his life had been spared for the more difficult problems of our day. If in our hours of struggle with the mysteries of nature these are our leaders and companions, so in our hours of ease and relaxation do we not turn

[1] By Henry Fairfield Osborn, LL.D., Hon.D.Sc. Camb., Da Costa Professor of Zoology, Columbia University, President of the American Museum of Natural History.

again to sons of Cambridge for spiritual refreshment, to the verse of Milton, of Byron, of Wordsworth and Tennyson, all richly imbued with the nature spirit, or to the no less masterly prose of Thackeray and Macaulay?

Far away are the giant forces of our republic, the roar of her machinery and her world of trade, yet the independence of her development is more apparent than real. There still prevails the potent unifying influence of mind and motive, bred in quiet places like this, ever creating the new generations of leaders in science, in literature and in government, and ever renewing the strong bonds of friendship and of union.

What can we add to the chorus of appreciation of the great pupil of Christ's which has come from college, press and pulpit since the opening of this anniversary year? Only a few words of personal impression.

To us, Darwin, more perhaps than any other naturalist, seems greatest in the union of a high order of genius with rare simplicity and transparency of thought. Dwelling on this lucid quality and on the vast range of his observation from the most minute to the grandest relations in nature, does not the image arise of a perfected optical instrument in which all personal equation, aberration and refraction is eliminated and through which, as it were, we gaze with a new vision into the marvelous forms and processes of the living world. With this wondrous lens our countrymen, Cope and Marsh, penetrated far deeper into fossil life than their predecessor Joseph Leidy—thus the arid deserts of the Rocky Mountain region gave up their petrified dead as proofs of Darwinism. Through its new powers Hyatt, Morse, Packard and Brooks saw far more than their master Louis Agassiz, and drew fresh testimonies of development from the historic waters of New England. From the very end of the new world, where the youthful Darwin received his first impressions of the mutability of the forms of life, we enjoy a clearer vision of the ancient life of Patagonia.

What of Darwin's future influence?

While it is doubtful if human speculation about life can ever again be so tangential as in our pre-Darwinian past of fifty years ago, it is probable, in fact it is daily becoming more evident, that the destiny of speculation is less the tangent than the maze—the maze of innumerable lesser principles, with as many prophets calling to us to seek this turning or that. There are those who in loyal advocacy of his system feel that we shall not get much nearer to life than Darwin did, but this is to abandon his progressive leadership, for if ever a master defined the unknown and pointed the way of investigation, certainly it was Darwin. In the wonderful round of addresses in his honor of this Centennial Year, and in the renewed critical study of his life and writings—the recognition that Darwin opened the way has come to many with the force of a fresh discovery. It is true that he left a system, and that he loved it as his own, but his forceful, self-unsparing and suggestive criticism show that if he were living in these days of Waagen, of Weismann, of Mendel and of De Vries, he would be in the front line of inquiry, armed with matchless assemblage of fact, with experiment and verification, and not least with incomparable candor and good will. This bequest of a noble method is hardly less precious than the immortal content of the "Origin of Species" itself.

In conclusion, we delegates, naturalists and friends, desire to present to Christ's College, as a memorial of our visit, a portrait of Charles Darwin in bronze, the work of our countryman, William Couper, a portrait which we trust will convey to this and future generations of Cambridge students, some impression of the rugged simplicity, as well as of the intellectual grandeur, of the man we revere and honor.

PERIDERMIUM STROBI KLEBAHN IN AMERICA

During the past few years several millions of young trees of white pine (*Pinus Strobus*) have been imported from Europe and distributed in the northeastern states. This has been done in spite of the obvious danger of bringing in insect pests and the fungus *Peridermium Strobi* Klebahn. The latter is not

known to occur in America, but is generally distributed in Europe, where in certain sections it is very destructive. It is mentioned as being abundant about Hamburg, where are located some of the largest forest tree nurseries of the world.

The writer has repeatedly examined imported white pine trees, anticipating the introduction of this disease, and, on June 8, succeeded in finding it. This is its first reported occurrence on white pine in America. Continued investigations now show that *Peridermium Strobi* is present on imported stock in the states of New York, Vermont, Massachusetts and Connecticut. The stock was imported from the nurseries of J. Heins Söhne, near Hamburg, Germany. All of the stock found infected thus far is three years old, that is, the seed was sown in 1906. There can be no doubt that it was infected in the German nurseries, as (1) the disease was not known to occur in America on the pine previous to this date, and in no form whatever except as mentioned below; (2) fruiting pustules have been found on three-year-old trees which were imported this spring; (3) fruiting pustules have also been found on trees which were imported in the spring of 1908 and placed in transplant beds in this country.

It is a well-known fact that a fungus disease introduced into a new climate is usually much more virulent in its attacks than in its native country. This fungus prevents the cultivation of *Pinus Strobus* in certain sections of Europe where the fungus is indigenous. We can hardly doubt that it will be even more destructive should it once obtain a foothold in America. The white pine is the tree upon which depends the entire reforestation movement which has been developed in the northeastern states. *Peridermium Strobi* threatens the profitable use of this species and thus directly concerns all who are connected with the reforestation question in this section. At the present stage in this movement it would be a national calamity to allow this fungus to become established in America.

Peridermium Strobi has an alternate stage on various species of *Ribes,* which is known as *Cronartium ribicola* Fisch. de Waldh. The fungus probably will not transfer from white pine to white pine, but must pass from white pine to *Ribes,* and vice versa. The *Cronartium* stage was found established in New York in 1906 by Stewart,[1] who apparently was entirely successful in eradicating it. The *Peridermium* stage has never been previously found in this country, so far as now known. The best available method of treatment is (1) to immediately burn the diseased white pine trees; (2) to inspect all neighboring currant and gooseberry bushes in August and September, and to burn any affected plant; (3) to inspect the pine trees again in April or May of next spring (1910) and burn all that are found diseased. It may be also necessary to repeat the inspection of the currant bushes in 1910.

On June 28 a meeting was held in New York City of the representatives of the forest commissions of the states of New Hampshire, Vermont, Massachusetts, Connecticut, New York and New Jersey, the Pennsylvania and the Delaware and Hudson Railroads, the New York State Department of Agriculture, the Geneva Agricultural Experiment Station, the Yale Forest School, the Forest Service and the Bureau of Plant Industry of the United States Department of Agriculture. Concerted action along the lines recommended above was pledged by all concerned, and measures for preventing the further importation of diseased trees were taken under consideration.

PERLEY SPAULDING
BUREAU OF PLANT INDUSTRY

SCIENTIFIC NOTES AND NEWS

IT is scarcely necessary to remind readers of SCIENCE that the Winnipeg meeting of the British Association for the Advancement of Science opens on August 25, and that the council of the association has invited members of the American Association for the Advancement of Science to become members for the meeting. Abstracts of papers intended for presentation should be forwarded to the local secretaries, University of Manitoba.

[1] Stewart, F. C., Tech. Bull. N. Y. (Geneva) Agr. Expt. Station, 2: 60–74, 1906.

The French Association for the Advancement of Science will meet this year at Lille on August 2–7, under the presidency of Professor Landouzy, dean of the faculty of medicine in the University of Paris. The gold medal of the association, which was instituted last year, is to be awarded to Professor H. Poincaré, who will deliver a lecture during the course of the meeting.

The sixth International Congress of Psychology met at Geneva last week. Among the Americans whose names appear on the program are Professor J. Mark Baldwin, Professor Jacques Loeb, of the University of California; Professor H. S. Jennings, of the Johns Hopkins University; Professor Lightner Witmer, of the University of Pennsylvania; Professor James H. Leuba, of Bryn Mawr College; Professor Max Meyer, of the University of Missouri; Dr. R. M. Ogden, of the University of Tennessee; Professor J. W. Riley, of Vassar College, and Professor R. M. Yerkes, of Harvard University. According to a cablegram to the daily papers the seventh International Congress will meet at Boston, with Professor William James, of Harvard University, as honorary president; Professor J. Mark Baldwin, as president, and Professor E. B. Titchener, of Cornell University, and Professor J. McKeen Cattell, of Columbia University, as vice-presidents.

In recognition of services rendered to the cause of aerial navigation, the French government has conferred the decoration of the Legion of Honor upon Mr. Wilbur Wright, Mr. Orville Wright and M. Farman, and the Officer's Cross of the same order upon Mr. Hart O. Berg and M. Santos Dumont.

The Italian government has decorated Dr. Louis Borsch, an American physician in Paris, with the royal medal of the Ministry of Public Instruction on the recommendation of the committee of the International Congress of Ophthalmology.

Dr. C. Lloyd Morgan, F.R.S., has resigned the office of vice chancellor of the University of Bristol. In accepting the same the council has placed on record its sense of the distinguished services rendered by him to the cause of university education during the twenty-two years of his tenure of office.

Surgeon-General W. L. Gubbins, C.B., M.V.O., deputy director-general of the British Army Medical Service, has been appointed director-general in succession to Surgeon-General Sir A. Keogh, K.C.B.

The State Board of Health of South Carolina has opened a laboratory at Columbia under the direction of Dr. Francis A. Coward.

The New York State Department of Agriculture has been notified that two of its veterinarians, Drs. George R. Martin and John T. Hart, of Goshen, have contracted anthrax, while endeavoring to stamp out the disease among cattle in Orange County.

Professor F. W. Woll, in charge of the department of feed and fertilizer inspection of the College of Agriculture, University of Wisconsin, has left for the west to attend the meeting of the American Association of Agricultural Chemists at Denver from August 26 to 28, and the American Association of Agricultural Colleges and Experiment Stations at Seattle.

Dr. Andrew M. Soule, dean of the Agricultural College of the University of Georgia, and Dr. T. H. McHatton, professor of horticulture, will represent the University of Georgia at the Portland meeting of the Society for the Promotion of Agricultural Science and at the other scientific meetings to be held on the Pacific coast.

Dean H. L. Russell, of the College of Agriculture of the University of Wisconsin, is making a tour of several western states to visit colleges of agriculture and important farming sections, as well as to attend the scientific meetings to be held in connection with the Alaskan-Yukon Exposition. He will address the American Association of Farmers' Institute Workers at Seattle on "The Advisability of holding Demonstration Institutes in the Summer."

Professor Lewis M. Haupt, A.M., Sc.D., addressed the International Congress of Women held at Toronto, Canada, on June 29,

1909, on "Relief Measures in Industrial Crises."

THE following doctorates are reported by *Nature* to have been conferred by the University of London upon internal and external students for the theses mentioned and other papers: Mr. P. Hartley, "On the Nature of the Fat contained in the Liver, Kidney and Heart"; Mr. E. T. Mellor, "The Geology of the Neighborhood of Middelburg, etc."; Mr. J. Stephenson, "Studies on the Aquatic Oligochæta of the Punjab"; Mr. W. Makower, "On the Active Deposit of Radium"; and Mr. H. Stansfield, "The Echelon Spectroscope, its Secondary Action and the Structure of the Green Mercury Line."

IT is stated in the *Nation* that the publication of the complete works of Alessandro Volta is now assured by the action of the Italian government in contributing $3,000 towards the necessary expenses. A committee appointed jointly by the Reale Instituto Lombardo delle Scienze and the Reale Accademia dei Lincei will have charge of this edition, which, it is expected, will consist of five volumes, and be finished within two years.

WE learn from the *Journal* of the American Medical Association that the dedication of the monument in the court of honor of the College of Medicine of Paris to Professor Brouardel, the lamented dean of the college, took place on July 20. The monument, by the sculptor Denys Puech, consists of a stela on which is the marble bust of Brouardel, wearing the insignia of his office. At the foot are two allegorical figures representing hygiene and legal medicine.

DR. ROBERT EDWARD CARTER STEARNS, known for his work on the geographical distribution and variation of mollusca and for other work in natural science, honorary associate in zoology of the U. S. National Museum, has died at Los Angeles, in his eighty-third year.

DR. JOSEPH FREDERICK WHITEAVES, paleontologist and zoologist of the Canadian Geological Survey, died at Ottawa, on August 8, in his seventy-fourth year.

CAPTAIN ENGELSTAD, of the Norwegian Navy, was taking meteorological observations during a thunderstorm, on July 24, when he happened to touch the winch holding the copper wire attached to the kite, which was a thousand yards high, and was struck dead.

THE death was also announced of Professor G. Arth, professor of industrial chemistry in the University of Nancy, and of Dr. Eugen von Gothard, the Hungarian astronomer.

IT is proposed to invite the British Association to meet in Australia in 1913. The University of Melbourne is communicating with the different Australian universities with a view to formulating definite proposals. It is suggested that the invitation should proceed from the commonwealth.

THE trustees of the fund of $1,250,000 left by Henry Barnato to found a hospital in memory of his brother Barney Barnato and his cousin, Woolf Joel, have decided to devote the sum to the building and endowing of a cancer hospital in London.

THE sum of £6,500 will be appropriated by the British government to the Royal Society for the expenses of the aeronautical section of the National Physical Laboratory.

THE daily papers report that a reflecting telescope with a mirror of forty inches has been shipped from Cambridge to Flagstaff Observatory for Mr. Percival Lowell's Observatory. It is designed especially for planetary photography and will first be used in photographing Mars when the planet will be nearest the earth next month.

THE Field Museum of Natural History has secured, for its botanical department, the private herbarium of Dr. J. T. Rothrock, of Pennsylvania. This is the last private herbarium covering the exploration period of North America and contains very full and valuable series of the early collections of the great west, Mexico and Florida. Dr. Rothrock having been the botanist of the survey of the territories and an intimate of Dr. Asa Gray, Dr. Torrey, Dr. Thurber and other early botanists, he was able to secure a large amount of material covering the period dating from 1840 to 1880. The herbarium is especially rich in types and co-types of the plants of western North America.

The twelfth International Congress on Alcoholism, held in London last month, was attended by about 1,400 members, including 400 delegates from abroad.

The Kappa Chapter of the Alpha Chi Sigma Chemical Fraternity was installed at the University of Kansas on May 29. The chapter was installed by Dr. J. H. Mathews, of the University of Wisconsin, and Mr. L. S. Palmer, of the University of Missouri. The following men constitute the charter members of the new chapter: F. P. Brock, A. N. Budd, F. W. Bruckmiller, M. L. Breidenthal, H. N. Calderwood, P. V. Faragher, B. C. Frichot, Chas. Hoffman, H. A. Kohman, P. R. Parmelee, F. W. Padgett, E. R. Weidlein, G. S. Weith and A. J. Weith.

The expedition which is carrying relief supplies to Commander Robert E. Peary left St. John's on August 3. The 88-ton schooner *Jeanie* will take fifty tons of coal and the same amount of stores, which she will land at Etah, Greenland, to supplement the supplies on Peary's steamer *Roosevelt*. The schooner will return as soon as she has discharged her cargo, bringing any despatches Peary may have left.

The first section of the electric funicular railway from Le Fayet to the summit of Mont Blanc was opened to the public on July 25 as far as the Col de Voza (5,495 feet in height). In the morning the first train carried the local French authorities and engineers to the Col, covering the 7½ kilometers in 56 minutes. The whole of the line is constructed in the open. Work will shortly be commenced on the second and most difficult section, about eight kilometers long, to Tête Rousse (10,300 feet).

The Black Hills of South Dakota contain deposits of ores of the rare metals tin, tungsten and tantalum, which have been examined by Frank L. Hess, geologist, of the United States Geological Survey, whose report forms a paper in the Survey's Bulletin 380. The lack of commercially valuable tin deposits in the United States gave especial interest to the discovery of tin ores in the Black Hills. The deposits occur in the northern part of the hills at Tinton and in the southern part near Hill City, Keystone, Oreville and Custer. Mr. Hess sketches the geology of the tin deposits as exposed at the various mines and claims and gives brief accounts of their commercial development. Tungsten deposits occur in the Black Hills at several places, but have been exploited commercially only at Lead, in the central hills. The valuable ore is wolframite. Tungsten is of especial interest and value in connection with its use in tool steel and as a filament in incandescent electric lamps. The tantalum of the Black Hills occurs in the mineral columbite. The known deposits of columbite in the region are described in detail by Mr. Hess. Tantalum is also used in making filaments for incandescent electric lamps.

UNIVERSITY AND EDUCATIONAL NEWS

Kenyon College has recently received $100,000 from Samuel Mather, of Cleveland, the income of which is to be devoted to the increase of salaries of the faculty.

It is announced that the George Peabody College for Teachers will be erected in close proximity to Vanderbilt University and will be affiliated with it.

Dean Harry B. Hutchins has been appointed acting president of the University of Michigan, and will assume his office when President Angell's resignation takes effect on October 1.

Professor F. B. Mumford has been elected dean of the agricultural college in the University of Missouri to succeed Dean J. H. Waters, who has become president of the Kansas Agricultural College.

Dr. John B. Powers has been elected dean of the medical department of Wake Forest University, *vice* Dr. Watson S. Pankin, who resigned to become secretary of the state Board of Health.

The chair of plant pathology recently established in the University of Wisconsin College of Agriculture by legislative action has just been filled by the appointment by the regents of Dr. L. R. Jones, of the University

of Vermont. Professor Jones is a native of Wisconsin, who did his undergraduate work at Ripon College and later took his bachelor's degree at the University of Michigan. He spent three years in graduate study at Michigan and took his doctor's degree in 1894. After receiving his bachelor's degree he was appointed botanist at the University of Vermont, which position he has held continuously since 1889.

SIR ISAMBARD OWEN, principal of Armstrong College, Newcastle-on-Tyne, has been elected vice-chancellor of the University of Bristol and Professor J. Michell Clarke pro-vice-chancellor.

DISCUSSION AND CORRESPONDENCE

AMERICAN MEN OF SCIENCE AND THE QUESTION OF HEREDITY

TO THE EDITOR OF SCIENCE: The statement of Mr. W. J. Spillman in your issue of February 12 regarding the superiority of country-bred boys, which I contraverted in your issue of April 9 by an appeal to "Who's Who in America," led me to examine the data which Professor Cattell collected for his "Statistical Study of American Men of Science."[1]

I pointed out in my former letter that Professor Cattell found a marked superiority for cities over the rural districts in the production of men of scientific merit, while my own investigation shows that this may be extended to include leadership in various phases of activity.

Professor Cattell, moreover, discusses his data in relation to their bearing on the question of the inheritance of scientific aptitude. I should like in this letter to make a few points of criticism concerning his interpretation of his results. Although he calls attention to the ambiguity and insufficiency of certain of his figures, he nevertheless gives the impression that he considers his results in general an argument against heredity. For instance, he states (page 734) that

> The inequality in the production of scientific men in different parts of the country seems to be a forcible argument against the view of Dr. Galton and Professor Pearson that scientific performance is almost exclusively due to heredity.[2] It is unlikely that there are such differences in family stocks as would lead one part of the country to produce a hundred times as many scientific men as other parts. [This is one of the points I wish to criticize].

Also on page 735 Professor Cattell writes:

> The fact that there is not a significant difference in the average standing of scientific men born in different regions of the country tends to support the conclusion that scientific performance is mainly due to environment rather than to innate aptitude. If the fact that Massachusetts has produced relatively to its population four times as many scientific men as Pennsylvania and fifty times as many as the southern states were due to a superior stock, then we should expect that the average standing of its scientific men would be higher than elsewhere; but this is not the case. [The above sentence expresses the second point that I should like here to criticize.] Like most arguments intended to disentangle the complex factors "nature and nurture," this however is not conclusive. If scientific ability were innate, each tending to reach his level in spite of environment, then a potentially great man of science would become such wherever born, and we might expect a favorable environment to produce mediocre men, but not great men. But this argument is answered by the small number of scientific men from certain regions of the country. Differences in stock can scarcely be great enough to account for this; it seems to be due to circumstance. A further analysis of the curves of distribution might throw light on the problem. Thus it might be that the men of greatest genius were independent of the environment, while men of fair average performance were produced by it. Examples might be given in favor of this view, but I can not see that it is supported by the forms of the curves of distribution. I hope at some time to take up the question from a study of individual cases, but I have not as yet the data at hand. My general impression is that certain aptitudes, as for mathematics and music, are mainly innate, and

[1] SCIENCE, N. S., Vol. XXIV., No. 623, December 7, 1907.

[2] I should like to ask in passing for the exact references to the writings of these gentlemen in which they have stated that *scientific performance* is almost exclusively due to heredity, or words to this effect.

that kinds of character and degrees of ability are mainly innate, but that the direction of the performance is mainly due to circumstances, and that the environment imposes a veto on any performance not congenial to it.

Thus while Professor Cattell certainly is not dogmatic, there are two points which to him indicate that heredity can not be the chief cause of scientific performance. These are, first, the great disproportion in the birth rate of scientific men in some regions as compared with others, and, second, the failure of Massachusetts to have produced men of high average standing.

That one part of the country should produce a hundred times as many scientific men as another, or even fifty times as many, seems extraordinary from any point of view, and perhaps to some it would seem as unlikely from the standpoint of environment as from any other; but the point I wish to bring out is that the more probable significant disproportion lies a great deal lower than this. His figures, on page 733, show that the hundred-to-one ratio applies only where the data are very meager numerically, so that the probable error is necessarily large. In order to increase our totals and decrease our probable error, it is better to average a little belt of the southern states all of which show a very low ratio. Thus we get a close idea of the contrast between Massachusetts and a typical low-ratio southern state. The section comprised of the states North Carolina, South Carolina, Georgia, Tennessee, Alabama, Mississippi, Louisiana and Texas brings in 26 scientific men against 134 from Massachusetts, with an average ratio of 3.75 per million for the south against 108.8 per million for Massachusetts. Thus we find that Massachusetts has produced nearer to twenty-nine times as many, instead of one hundred times, or fifty times as many, which were the ratios mentioned by Professor Cattell.

This is still a wide disproportion and one that undoubtedly means something, but I personally should feel that we were getting closer to its real significance if we nearly double the ratio for all the southern states, on account of the negro population, and make it a question of northern whites against southern whites. By such a method we get a more homogeneous mass of material, a desirability in biometrical work.

According to Professor Cattell "the negro may have a racial disqualification [for scientific achievement], but even this is not proved." It is of course impossible to absolutely disprove such a disqualification, but the same might be said for any organisms, no matter how low in the scale of mental evolution. The fact that millions of negroes have been to school and yet one would scarcely know where to find a single example of a negro scientist suggests strongly an experiment of millions of trials and millions of failures, which gives us some idea of its probability. Is not probability all that we can get out of statistics, anyway? If we adopt the method of leaving the negroes out of the ratios, we find that Massachusetts has produced more probably about seventeen times as many scientific men as a low-ratio southern state. That is, the ratio for the average southern state is raised from 3.75 per million to 6.54 per million, while the ratio for Massachusetts is merely raised from 108.8 to 109.7 per million.

There were 27,001,491 whites in the United States according to the census of 1860, and 1,221,464 of these lived in Massachusetts. Thus this state might have been expected to have produced 4.52 per cent. of the men of science in Professor Cattell's list. As a matter of fact it has produced 15.4 per cent., or 3.4 times the expected.

Now, the interesting question arises—Is this discrepancy more than might be reasonably accounted for by differences in stock? I know of no way of exactly answering this question, but I should like to make record here of some investigations I have carried on which seem to show that the results of Professor Cattell may very likely be entirely due to differences in stock.

First, let us see how Massachusetts stands when general intellectual achievement is taken into consideration instead of special merit in science alone. A little computation from the birth statistics in the latest issue of "Who's Who in America" shows that taking the cen-

sus of 1860 as our basis, Massachusetts has produced 11.6 per cent. of the total, or 2.6 times the expected. This is not as great a disproportion as one finds in Professor Cattell's statistics, but it is a high one. Massachusetts leads all other states, and is easily ten to thirty times ahead of some states. Furthermore, it is almost certain that the standard represented by Professor Cattell's list of one thousand is somewhat higher than that represented by admission into "Who's Who in America." This I assume to be the case because I have calculated that there are about two thousand names of scientific men in the latter volume who would be included under the various specialties tabulated in Professor Cattell's study.

One of the chief reasons which this investigator gives for doubting that his results are due to differences in stock is the fact that Massachusetts has not produced relatively more men of the highest grades in science. His figures are sixty in the total for the superior grades (I.–V.) and seventy-four in the less superior grades (VI.–X.). This would doubtless be a very strong argument in just the direction which he has indicated if it should be substantiated by further statistics; but I have several reasons for thinking that as the figures here stand, this unexpected ratio in the single case of Massachusetts is caused by the smallness of the figures themselves. I have arranged the states in two groups, so that one group, Massachusetts, Colorado, Connecticut, Washington, Nebraska, Kansas, Vermont, New York, Maine and New Hampshire, contains half the total number of scientists, and all the superior ratio states in the order of their superiority. Massachusetts then appears to be an exception, for the group as a whole *does* average more names in the I.–V. grades than in the VI.–X. The difference is, however, only a slight one, being 227 against 205. This is outside of, though not twice, the probable error 7, so the difference is suggestive if not significant, and the total number of cases is at the same time proved not great enough for a final conclusion. If, however, the same ratio were maintained for a greater mass of data, it would soon become significant as showing a higher average standing for scientists born in the high-ratio states. Thus if the totals were all raised a hundredfold, since the probable error would be increased only tenfold, the difference would then be fifteen times the probable error, and the chances against mere hazard's explaining the result would be enormous.

It may be that the actual intellectual differences between those in the I.–V. and those in the VI.–X. grades are really not very great after all. I fancy there are many more names of older men in the I.–V. or higher grades, who with the same amount of brains as many younger men in the lower grades have had a longer time in which to gain a reputation. Even so, the I.–V. grades should average somewhat above the VI.–X., in real ability, though it is easy to see that something of the true difference in actual merit between the two groups is lost as soon as differences in age-average exist between them. Such a force would work mathematically in two opposed directions and cause confusion. Without the age-averages of the two groups, it is impossible to say how this factor might affect the results.

I thought it would be interesting to know if the ratio of scientific men born in Massachusetts would be as high if all examples be taken into the discussion, using the entire book "American Men of Science" instead of the thousand selected and presumably superior group used by Professor Cattell. I have therefore had a count made of the entire number in this book of 4,000 names. There are 436 reported as born in Massachusetts. The exact total number of names is unfortunately not printed in the book, but is given as more than 4,000. Taking the number even as low as 4,000 and assuming that 87 per cent. were born in this country, as is the case with the 1,000 superior ones, the per cent. born in Massachusetts is reduced from 15.4, found for the superior group, to 12.5 for scientists of all degrees of merit. Thus there is something to show that Massachusetts *has* produced relatively more men of science of the superior sort.

Turning now from special aptitude in science to general mental eminence, as shown in

all fields of activity, let us see if there are any facts to indicate that the ratio of 11.6 per cent. for Massachusetts birth, or 2.6 times the expected, found for "Who's Who in America" is any higher if one takes a much more select group of names. I think of only two such lists already in existence. Both possess decided objective value. One is comprised of the names of the thirty Americans included in Profesor Cattell's "Statistical Study of Eminent Men." The other is the roll of thirty-seven in the "Hall of Fame."

I find that out of the thirty in Professor Cattell's list, eleven were born in Massachusetts, or 36.6 per cent. I have gone back to the first census of 1790 as being approximate to the time of their birth, on which basis there should have been about 12 per cent., or the ratio is about three times the expected. In the "Hall of Fame" I find fifteen born in Massachusetts, or 40.6 per cent. against about 12 per cent. expected. It will be observed that both these ratios are higher than the 2.6 times the expected found for the names in "Who's Who in America."

If the greatest eminence is more independent of environment than the lesser forms, why then should not heredity and environment working together produce a higher ratio for Massachusetts when the lesser standard is taken than when only the truly remarkable are concerned? If these illustrious characters are born such and not made, we get some suggestion of how really superior the stock of Massachusetts must be as compared with any other part of the country. It can be seen from the foot-note[3] that with the exception of Virginia the entire country to the south of New York has done almost nothing in producing our greatest Americans. If Massachusetts has given birth to seventeen times as many men of especial scientific merit as some other sections of the country, she has at the same time produced more nearly fifty or a hundred times as many men, if the highest ranks of eminence be alone considered.

There still remains, I think, something from the various figures that I have so far analyzed to indicate that New England, and especially Massachusetts, shows a slightly higher aptitude for science than for general intellectual performance taken as a whole. But is this more than might be expected from differences traceable to selection of stocks, to differences in types of mind in those who emigrated to the various colonies? I can not, of course, answer this question. It is, however, the general impression that the south was peopled, aside from the negroes, by two classes, the gentry and the poor whites. The descendants of the cavaliers were people of refinement and polish, rather inclined to hospitality and good living, with interests of a practical, legal and political sort, than to the serious contemplation which is supposed to have characterized the puritans. As for the poor whites of the south, they are certainly not the stock from which one would expect scientists.

The factor "density of population" which Professor Cattell mentions first under his "main factors in producing scientific and other forms of intellectual performance" deserves, I think, a slight criticism. In the first place we do not find the center for the birth of scientific men (which is around Massachusetts or Connecticut) at all coinciding with the general population center, which in 1860 was twenty miles south of Chillicothe, Ohio. Moreover, a list of the states according to density of population at that time, gives us, District of Columbia, Rhode Island, Massachusetts, Connecticut, New Jersey, New York, Maryland, Pennsylvania, Ohio, Delaware as the ten leading

[3] "*Eminent Men*": Massachusetts, 10; Virginia, 7; New York, 3; Ohio, 2; Rhode Island, Maine, New Hampshire, New Jersey, Pennsylvania, Kentucky, Tennessee and Louisiana, each 1; Vermont, Connecticut, Delaware, Maryland, North Carolina, South Carolina and Georgia, none.

"*Hall of Fame*": Massachusetts, 14; Virginia, 6; New York, 4; Connecticut, 3; Rhode Island, 2; Ohio, 2; Maine, New Hampshire, Pennsylvania, Kentucky, Tennessee and Louisiana, each 1; Vermont, Delaware, Maryland, North Carolina, South Carolina and Georgia, none.

It can be seen that, with the exception of Connecticut, the proportionate agreement between these two lists, formed by entirely different methods, is almost perfect.

states for density. One finds in this list only three of Professor Cattell's ten leading states. that is, the ten leading states which gave birth to half his total number of scientists. Should the problem be worked out carefully there would be found, I have no doubt, some correlation between the birth of superior men and density of population. Considering the great over-proportion which cities are known to produce, I can not see how it can fail to be so, but it appears on first sight that it will be significant to one who might wish to predict a result, not so much to know that there is a center of density as to know which particular center it is. The group of states, New Jersey, Pennsylvania, Delaware, Maryland and Virginia, are not usually thought of as lacking in "wealth, opportunity, institutions and social traditions," and yet this territory is distinctly behind New England in the production of scientific men, and only as good as the great western and north-central divisions, which were largely peopled by New England stock.

One might ask why the latter districts, if formed from the stock of New England, have not done equally as well as New England itself. The answer from the standpoint of heredity would be that distinguished scientific men come in great proportion from families of the professional and upper classes[4] and that these families had, prior to 1860, generally stayed at home in New England. The great western migration of the last century must have produced a kind of natural selection. Very likely the west has been the gainer and New England the loser, from the standpoint of vigor, energy and ambition. But it seems fair to suppose that while the better of the middle classes might have joined the emigrant trains, the intellectual aristocracy did not.

To distinguish between heredity and environment is at best a difficult problem, and the statistics here analyzed give, of course, no final answer. All I wish to say is, that there is nothing in these birth ratios to shake ones belief in the extreme importance of heredity,[5] or even to show that environment is the main cause of the "direction of the performance" itself.

[4] Conf. Galton, "English Men of Science," London, 1874; Galton and Schuster, "Noteworthy Families," London, 1906; Candolle, "Histoire des sciences et des savants," Genève, 1873; Ellis, "A Study of British Genius," London, 1904.

FREDERICK ADAMS WOODS
BROOKLINE, MASS.,
April 15, 1909

DR. WOODS permits me to add some comments to his discussion. The adjacent states of Massachusetts and Connecticut, with a population of 1,691,213 in 1860, have produced 174 of our thousand leading scientific men, whereas the adjacent states of Georgia, Florida, Alabama, Mississippi and Louisiana, with a population of 3,661,218 in 1860, have produced but seven. The one region has produced per thousand of its population more than fifty times as many scientific men as the other.[1] This great difference, it appears, is more probably due to social conditions, educa-

[1] This disparity will be reduced to nearly half it the negroes are excluded. The fact that the southern whites are nearer to the negroes in their scientific productivity than to New Englanders, is in favor of scientific performance being due to social environment rather than to stock. A similar argument may be drawn from the fact, if it proves to be a fact, that mulattoes resemble blacks more than whites in their scientific productivity. It is, however, also the case that if the southern whites and the negroes were given equally an environment favorable to scientific work, the whites might far surpass the negroes. The question as to whether scientific productivity is mainly due to heredity or environment is not one that can be answered without qualifications and explanations. If environment is the same, differences are due to heredity; if heredity is the same, differences are due to environment. As President Lowell has recently remarked, we have a better chance of rearing eaglets from eagles' eggs placed under a hen than from hens' eggs placed in an eagle's nest. But it is also true that we have a better chance of raising tame eaglets in a chicken coop than in an eyrie. The difference between a man uninterested in science and a scientific man is not that between a chicken and an eagle, but that between an untrained chicken and a trick

[5] For a list of researches which lead to this belief, see SCIENCE, April 9, 1909, page 579.

tional institutions and opportunities for a career than to stock, and is thus evidence in favor of scientific productivity being in the main due to opportunity rather than to heredity. It is probable that if the 174 babies born in New England who became leading scientific men had been exchanged with babies born in the south, the scientific productivity of New England would not in that generation have been materially decreased, nor the scientific productivity of the south have been greatly increased. It is certain that there would not have been 174 leading scientific men from the extreme southern states and only seven from Massachusetts and Connecticut. If the stock of the southern states remains undiluted, it may, as social conditions change, produce even more scientific men per thousand of its population than New England has hitherto produced. Japan had no scientific men a generation ago and China has none now, but it may be that in a few years their contributions to science will rival ours.

The second point discussed by Dr. Woods is my qualified inference that the fact that those regions which have produced more scientific men have not produced men of higher average performance is against the theory that scientific productivity is mainly due to heredity. Dr. Woods says that this would doubtless be a very strong argument if it should be substantiated by further statistics. His discussion of my statistics does not seem to alter the interpretation put on them. He, however, brings forward new data of interest, which show that the scientific men produced by Massachusetts are slightly above the average and that Massachusetts has produced far more than its share of men of unusual eminence. These facts do not, however, affect my argument. It would be expected that the educational advantages and opportunities for research in Massachusetts would give its scientific men a higher average standing than those elsewhere, even though their native ability were the same. It is surprising that this does not show at all in the 1,000 leading men of science and but slightly in the 4,131 included in the "Biographical Directory." In the case of men of exceptional genius, I agree with Dr. Woods that they can not be regarded as the product of their environment. But it may interpose a veto on their performances. There may be "mute inglorious" Emersons in southern churchyards. Lincoln was as great a writer as Emerson; but it is in a way a chance that he made his Gettysburg speech. It is likely, but not proved, that one region of this country or one of its racial stocks has more potential men of genius than another.

While views such as those of Dr. Galton when he says "The impression that all this evidence leaves on the mind is one of some wonder whether nurture can do anything at all" or of Professor Pearson when he says "We inherit our parents' tempers, our parents' conscientiousness, shyness and ability, even [to the same extent] as we inherit their stature, forearm and span," seem to be extreme, I hold, as stated in the paper quoted by Dr. Woods, that "kinds of character and degrees of ability are mainly innate." But I believe also that there is in this country a vast amount of the character and ability required for scientific productivity which is not used for this purpose, and that the quantity, though not the quality, of our scientific work could be increased to almost any extent. What a man can do is prescribed by heredity; what he does is determined by circumstance.

J. McKeen Cattell

cock. Some cockerells can be trained better than others, but there are innumerable cockerells that might be trained and are not.

GENERA WITHOUT SPECIES

The views on genera without species held by Dr. J. A. Allen, as expressed in Science, June 11, 1909, may possibly be shared by a few entomologists interested in restricted groups and by many students of higher forms of life, such as birds and animals. It is not remarkable that an ornithologist or mammalogist, whose entire number of subjects scarcely equals that of the species of a single family of some orders of insects, should hold that personal judgment should enter into the solving of this important problem. It is the man

who is concerned with the hundreds of thousands of names rather than he who deals with the thousands that sees most clearly the hopelessness of gaining stability by methods where personal opinion is given full sway. Dr. Allen attributes the remarkable unanimity of opinion of those opposed to his views to inexperience or ignorance of the subject. If the worthy doctor himself was more experienced in the fields of entomology or botany, where the forms are countless as compared with animals or birds, he might be less positive in his position on this question. It may not be absurd to state that an ornithological genus based on an unnamed woodpecker with three toes can not be mistaken because but one such bird was known. But would it seem so plausible to state that a genus of insects based on an unnamed specimen of parasitic hymenoptera, or a minute fly, with a certain vein of the wing forked before the middle was unmistakable because but one such species was known while many thousands of such little creatures are flying undescribed about us?

It is true that this question is not definitely covered by the International Code, but certain statements do have a bearing on the subject. On page 11 of the code the generic and specific name is likened to the family and individual names of persons. Now who can conceive of a family of Smiths without a John or a Jane in it? Would it not seem silly to have a name Johnson before any one was born to bear it? Getting back to genera, what is a genus? "An aggregation of one or more species" would seem to be a good definition. If such a definition was accepted it would certainly invalidate the genus without species, so I presume Dr. Allen has another definition. Not knowing what it is, I can not discuss it. The code does not define the genus. However, it is now quite universally agreed that a genus should have a type designated. Article 30 of the code, paragraph 2, says: ". . . nor can a species be selected as type which was not originally included in the genus. . . ." This being true, how can we get a type for a genus where there were no species originally included? In the amendments to the code, published in SCIENCE for October 8, 1907, is the following: "The commission is unanimously of the opinion that a *name,* in the sense of the code, refers to the designation by which the actual objects are known." Now a genus without a species has no object; it is a name applied to a conception, not to an object and can therefore have no place in systematic nomenclature.[1]

No one, I think, claims infallibility for the international code; but it is certainly not to the best interest of nomenclatorial stability to knowingly violate its recommendations. An able board of chosen nomenclaturists has passed on and sanctioned these rules and formulated them into an accessible code, and it should be incumbent upon systematists to comply with them so far as possible. There are enough questions not covered by the code to furnish constant contention without bringing up problems that are capable of being disposed of under the rules already formulated. That which is best in one group may not be the best for another, but for the sake of uniformity and in the hope of future stability let us accept the dictum of the International Zoological Congress and follow the code.

A. N. CAUDELL
U. S. NATIONAL MUSEUM

A NOTE ON UROPHLYCTIS ALFALFÆ (V. LAGERH.) P. MAGN. IN CALIFORNIA

A CROWN gall of alfalfa (*Medicago sativa*) which occurs in Europe, but which, so far as known by the writer, has not before been noted in this country, has recently come to our attention in California.

The disease was first observed in Ecuador in 1892 by Lagerheim, who placed the parasitic fungus causing it in the genus *Cladochytrium*. In 1902 it was found in Alsace, Germany, by Magnus, who transferred the organism to the genus *Urophlyctis*. It has since been observed in other localities on the continent, where it has done considerable damage.

The galls are usually very numerous at the

[1] *Nomen nudum* does not seem inappropriate in this connection.

crown of the affected plant, and frequently occur an inch or two up on the stem. Though usually small, or in an irregular divided mass, they may be round and unbroken, and three or four inches in diameter. The interior of the gall is composed of small, irregular cavities in the hypertrophied tissue, the chambers being filled with masses of brown resting spores about forty micro-millimeters in diameter.

A more detailed account of the disease as it occurs in California will be published shortly.

Elizabeth H. Smith
Department of Plant Pathology,
University of California,
June 2, 1909

THE WEST INDIAN SEAL AT THE AQUARIUM

The New York Aquarium received on June 14, 1909, an adult male and three yearling specimens of the rare West Indian seal (*Monachus tropicalis*). One of the latter was in a weak condition and died the day after arrival. The others are apparently doing well. The specimens were procured from a dealer in live turtles at Progresso, Yucutan, who reported the species as a great rarity. They were presumably captured at either the Triangle or the Alacran islets in the Gulf of Campeachy, the only known resorts of the species at the present time, so far as I am aware.

They are probably the only specimens of this nearly extinct species now living in captivity. Its original range included the coasts of Cuba, Haiti, Jamaica and the Bahamas. For the last half century it has apparently been restricted to the islands of Yucatan. It was well known to the sailors of Columbus and was later the basis of a seal fishery.

In Science for April 13, 1906, I recorded the killing of a specimen at Key West, Florida, on February 26, 1906. The species had not been seen in Florida for about thirty years.

The New York Aquarium received two specimens in 1897, one of which lived in the aquarium until 1903. Both of these animals had the singular habit of filling their cheeks with water and blowing it suddenly and with considerable force into the faces of visitors leaning over the pool. It will be interesting to discover whether the specimens now in the building develop this trick, which for years excited the amusement, and sometimes the wrath, of visitors. Unlike the other Phocidæ kept on exhibition here, *Monachus* is noisy, the young often roaring harshly.

C. H. Townsend
New York Aquarium

SCIENTIFIC BOOKS

Scientific Papers. By Sir George Howard Darwin. Cambridge, at the University Press. Vol. I., pp. xiv + 459; Vol. II., pp. xvi + 514.

The task of the reviewer who undertakes the consideration of a republication of matter which has been for some years before the public in an accessible form, is not particularly easy. He may, of course, take refuge in statements more or less detailed of the contents of the volumes before him and say little more than the intelligent reader can glean by turning over the pages himself. Or he may write a few paragraphs on the history of the subjects treated, showing the author's relation thereto and his place in their development. More often he seizes a few points of a controversial character and in discussing them simply adds to the literature of the subject. No one of these methods appears to be satisfactory from the point of view of the reader. Brief reviews not intended for serious study, although they may be the result of such study, should, it seems, be written chiefly to save time and labor for the reader and perhaps to express the opinions of the reviewer, since in scientific journals at least the editorial " we " has ceased to be even a disguised fiction. Such reviews thus necessarily pass into the class of ephemeral productions which may have value at the time of publication, but which only add to the labor of future students if they contain matter belonging properly to the development of the subject.

If a writer accepts this view and takes to the criticism of matters beyond the mechanical detail of form and arrangement, an estimate of the writer and his work, however dangerous, is necessarily the main topic. After all, such criticism is merely a single opinion as to whether the attitude of the scholar towards his

work, his methods and his manner should or should not be recommended for the guidance of those who wish to aim at similar achievements. This at any rate is true with respect to works like the one before us, their intrinsic value not being open to question. In reading such productions, especially in collected form, one necessarily forms a picture, conscious or otherwise, of the author's mind quite apart from any personal acquaintance, and it may be that no apology is needed for recording such an impression and for pointing out some features of the author's work and methods which appear to the present reviewer to be of high value in the study of applied mathematics.

The lines on which Sir George Darwin has so far published memoirs have been mainly laid in the subject of hydromechanics and its mathematical associate, the theory of elastic solids. These have been applied to masses in bulk and subject to the Newtonian law of gravitation with applications to present-day astronomical and geophysical problems and to theories of cosmogony on the basis of a continuous evolution of planetary and satellite systems. These problems, whether we consider them from the mathematical or physical side, are certainly the most difficult of all the studies in celestial mechanics which have received attention in the past. It is true that there are many ideal problems which admit of moderately simple treatment, but the majority of these can not be considered even as a first approximation to the real problems presented by the observed phenomena. One notices throughout Professor Darwin's work that he rarely takes up an ideal problem without having in view the ultimate answer to some physical question. Almost the single exception to this is his paper on periodic orbits, and the possible complications of solar systems other than that to which we belong makes this exception a doubtful one.

The difficulties are increased when it is desired either to interpret the mathematical results in terms of simple and easily understood phenomena or to put them into such forms that numerical values may be substituted directly for the symbols. These objects Professor Darwin appears to have continually in view. He is rarely contented with the mathematical solution of a set of differential equations, however intricate or interesting the solution may be from the symbolic point of view, but searches to find the physical explanation or analogy which will enable him and the reader to follow without great mental effort the various sequences of events which must result from the formulæ obtained.

One notices too the wide range between what are popularly called "practical results" and speculative theory. On the one hand, we have his work on tidal prediction with all its associated problems, solutions of which are necessary or valuable in the concerns of daily life, and on the other hand, the highly speculative (I use his own term) investigations into the past history of the satellites and planets which form our solar system. In the former case the object in view was chiefly the discovery of methods for obtaining the times and heights of the tides at any place without excessive labor, that is, in these days when numerical computation is a recognized profession, at a cost which is within the means of those who desire the results. In such work the mathematical developments have generally a minor interest: they are considered to be merely tools for the fashioning of the machine under construction. At the other end of the scale is the work of purely scientific interest related to problems of cosmogony, chiefly those of the past, in which the mathematical developments frequently have an interest of their own and give rise to problems in pure mathematics which may be entirely separated from any physical considerations.

In using the term "highly speculative" it is well to distinguish between work of the character of Darwin's theory of the lunar evolution based on known physical data clearly stated, developed with rigid mathematical accuracy and with careful attention to any factors which may modify the results, and the ill-defined guesses developed, it may be, from some isolated principle which not infrequently appear as proofs of the correctness or falsity of some new or old idea. In the matter of cosmogony this has been especially obvious of

late years, and nowhere more so than in the attempts to trace back astronomical systems to an earlier state on the theory of a general evolution of the arrangement of matter. One can not read Sir George Darwin's work in this line without being impressed with the caution which he continually exercises and wishes to impress on the reader for any results, especially those which are numerical, that he may print. It is interesting to learn his own opinion of the outcome of the controversies which have sprung from the theories set forth years ago in his papers, and I can not do better than to quote his estimate as given in the preface to Volume II. In referring to the papers on Tidal Friction and Cosmogony, which fill this volume, he says:

> For the astronomer who is interested in cosmogony the important point is the degree of applicability of the theory as a whole to celestial evolution. To me it seems that the theory has rather gained than lost in the esteem of men of science during the last twenty-five years, and I observe that several writers are disposed to accept it as an established acquisition to our knowledge of cosmogony.
>
> Undue weight has sometimes been laid on the exact numerical values assigned for defining the primitive configuration of the earth and moon. In so speculative a matter close accuracy is unattainable, for a different theory of frictionally retarded tides would inevitably lead to a slight difference in the conclusion; moreover, such a real cause as the secular increase in the masses of the earth and moon through the accumulation of meteoric dust, and possibly other causes are left out of consideration.
>
> The exact nature of the process by which the moon was detached from the earth must remain even more speculative. I suggested that the fission of the primitive planet may have been brought about by the synchronism of the solar tide with the period of the fundamental free oscillation of the planet, and the suggestion has received a degree of attention which I never anticipated. It may be that we shall never attain to a higher degree of certainty in these obscure questions than we now possess, but I would maintain that we may now hold with confidence that the moon originated by a process of fission from the primitive planet, that at first she revolved in an orbit close to the present surface of the earth, and that tidal friction has been the principal agent which transformed the system to its present configuration.

After some remarks on the difficulties in the way of the acceptance of the theory due to the time element and the probable date of solidification he refers to the arguments of Lord Kelvin based on temperature gradients and points out how the possible effects of radioactivity and the "colossal internal energy resident in the atom" may tend to nullify any estimates based on temperature alone. Summing up, he adds:

> It is very improbable that tidal friction has been the dominant cause of change in any of the other planetary sub-systems or in the solar system itself. Yet it seems to throw light on the distribution of the satellites amongst the several planets. It explains the identity of the rotation of the moon with her orbital motion, as was long ago pointed out by Kant and Laplace, and it tends to confirm the correctness of the observations according to which Venus always presents the same face to the sun.

The arrangement adopted for the order of the papers is not entirely chronological. Professor Darwin remarks that in the case of his own work, his papers fall into a few well-defined groups and that this fact furnished him with the opportunity to place together those papers which deal with particular subjects. As a result of this arrangement the first part of Volume I. consists of what is practically a treatise on the tides and tidal prediction, the second part containing two papers on the lunar disturbance of gravity, while the whole of Volume II. is devoted, as stated above, to tidal friction and cosmogony. Appended to each volume is a chronological list of all his papers with references to the volumes in which they are or will be contained.

The collection will thus be something more than a reprint. In general the reproduction is literal, but here and there are notes pointing out an occasional error or giving a reference to later work by himself or others which has tended to modify the earlier conclusions or to fill gaps which had existed. One sometimes wishes that these notes had been more frequent so as to include references and perhaps

brief accounts of later work. However, for the greater part of these two volumes, the article Bewegung der Hydrosphäre in Vol. VI., 1, 6, of the "Encyklopädie der mathematischen Wissenschaften," by Darwin and Hough, will be found to supply these needs. This article is to be reproduced, it is to be hoped in English, in Volume IV. of the "Scientific Papers."

I may mention one point in conclusion in connection with Sir George Darwin's presentation of his work which earns the gratitude of those who are unable from want of time or training to read his papers in detail as well as of those who do such reading but wish to get a general view of his processes and results as a first step. In the summaries to the longer memoirs he gives not only the general conclusions at which he has arrived, but also a brief account, without symbols, of the hypotheses on which the arguments are based, the methods employed and the general course of the mathematical procedure. These summaries have made the task of following his work very much less difficult and have doubtless contributed in some measure to the early acceptance of the theories which he has set forth.

The printing done by the Cambridge University Press is too well known to need comment here. The size of the volume adopted is the modern compromise between convenience for the printing of long formulæ and suitability of size for easy handling and reading, namely, the royal octavo between one and two inches thick.

ERNEST W. BROWN

The Problem of Age, Growth and Death: A Study of Cytomorphosis based on Lectures at the Lowell Institute, March, 1907. By CHARLES SEDGWICK MINOT. New York and London, G. P. Putnam's Sons. 1908. Pp. 280; good index.

Many biologists, and with them a wide circle of people who are intelligently interested in fundamental problems of life, have followed Professor Minot's researches in this field with keen interest for thirty years past, and will be glad to have the many scattered papers collected in book form and brought down to date. The purpose of the book is stated in the elaborate "introductory letter," which is addressed to Senator Mosso, to whom the work is inscribed. It is a study of "increase in the amount of protoplasm" as compared with the bulk of nucleus in the cells of the growing animal.

The first lecture deals with the process of growing old as seen in the body as a whole; and while the familiar data are exceptionally well presented, it calls for no special review. The second lecture, "Cytomorphosis—the Cellular Changes of Age," carries a somewhat parallel line of thought through the microscopical changes in the cells and tissues from the germinal to the senescent condition. Here we learn, in connection with appropriate figures in the text, about the "cytomorphic cycles" of different cells, connective tissue, nerve, muscle, gland and blood; and of the death and old age of cells in atrophy or degenerations of various kinds. As cells differentiate from the germinal to the adult form they become fitted to perform specialized functions, but lose the germinal power of growth and regeneration. Thus death is continually present in life, and may be even more active in the embryo, as, in the rapid whirl of cell-life, whole organs form and vanish, than during any other period of life. In fact one of the main theses is that: "The period of most rapid decline is youth; the period of slowest decline is old age."

The third lecture—"The Rate of Growth," gives the results of the author's extensive studies on the growth of guinea-pigs, rabbits and fowls and correlates them with those of Quetelet, Donaldson, Muhlmann and Thoma for man. The facts are presented with great precision in text, table, series of figures of embryos and by most striking charts and curves, growth being expressed, in the main, in percentage increments. The chief result is that power to grow is greatest in the germ and decreases rapidly with age. For example, using Richard Hertwig's calculation, the fertilized human ovum is 0.004 of a cubic millimeter; the child at birth from 3 to 4,000,000 cubic millimeters, which shows an increase of one billion times the original mass during gestation. From birth on to twenty years of

age the increase is only as 1 to 16. Thus Dr. Minot figures that: "Over 98 per cent. of the original growth power of the rabbit or chick has been lost at the time of birth or hatching," respectively, and the same thing is equally true of man. "We start out at birth certainly with less than two per cent. of the original growth power with which we are endowed."

While this conclusion is announced as most remarkable, and possibly it may seem so from the standpoint of an anatomist, the physiologist, and it would seem the biologist as well, would welcome it as a sign of efficiency. The quicker the machine can be built, adjusted and set to do the work designed for it the better. So the illustration which Dr. Minot chooses seems inapt. He says:

> But as that accumulation (of protoplasm) goes on, our body seems to become, as it were, tired. We may compare it to a man building a wall. He begins at first with great energy, full of vigor; the wall goes up rapidly; and as the labor continues fatigue comes into play. Moreover, the wall grows higher, and it takes more effort and time to carry the material up to its top, and to continue to raise its height, and so, as the wall grows higher and higher, it grows more slowly, and ever more slowly, because the obstacles to be overcome have increased with the very height of the wall itself. So it seems with the increase of the organism; with the increase of our development, the obstacles to our growth increase.

This statement of the case seems to be crucial to Dr. Minot's conception of the significance of growth and a biologist may be pardoned for wondering whether, even with so inapt a figure, the builder might not finish his wall and then use it, without fatigue; or whether, at certain stages in the work, he might stop building and begin chiseling inscriptions or ornaments upon it without fatigue. In reality we have, instead of a man building a wall of no definite height and for no definite use, an inventor building a machine, every dimension of which is fixed and subordinated to a definite purpose. It would be suicidal to go on "growing," building the machine larger after it is completed, and it is entirely conceivable biologically that the inventor might finish his machine and run it until it wears out or breaks down without any of the slowing down or growing tired of which our author makes so much. The wall simile must recall to the reader, the building of a certain tower, the completion of which was interdicted.

Special function presupposes specialized protoplasm, and we are next led to consider "Differentiation and Rejuvenation." The chief point in this field is that "*Rejuvenation is accomplished chiefly by the segmentation of the ovum.*" That is, in segmentation the cells are greatly diminished in size, with great increase in the amount of nucleus in proportion to protoplasm; and thus segmentation brings about the production of young cells.

The entomorphic cycle is thus again started, and as the cells differentiate and grow old it becomes impossible for them to become young again. The evidence is marshaled to prove that there is no "retrogressive development—Entdifferenzirung," as Driesch and Korschelt maintain. Instead of this certain cells retain the embryonic condition in all organs and tissues capable of regeneration and all such new growth is accomplished by proliferation of these young cells. Furthermore, the reproductive cells are early shunted out of the cytomorphic cycle of the individual, and, retaining the character of undifferentiated germ cells, are able to rejuvenate successive generations—the familiar "germ-plasm theory" commonly accredited to Wiesmann, but which Dr. Minot traces to Nussbaum. A new point of fundamental interest is raised in this connection, viz., the differentiation of nuclei; but while this is claimed we are disappointed that neither the figures nor the text makes it at all clear in exactly what changes this differentiation consists.

The final lecture on "The Four Laws of Age" gives the author's conclusion of the whole matter in categorical form.

> *First*, rejuvenation depends on the increase of the nuclei.
>
> *Second*, senescence depends on the increase of the protoplasm, and on the differentiation of the cells.
>
> *Third*, the rate of growth depends on the degree of senescence.

Fourth, senescence is at its maximum in the very young stages, and the rate of senescence diminishes with age.

As the corollary from these, we have this—natural death is the consequence of cellular differentiation.

Those interested in education will find in this last chapter a fine statement of rapid mental growth of the infant.

Clearly in ultimate analysis the growth of an organism is but a small factor in the problem of its entire life. At a guess, the energy expended in growth is to the entire energy output of the organism as one to one thousand. It should also be remembered in this connection that all the material which enables the embryo to grow so fast at first is a function of the adult life work of the parents. An adult hen may lay 200 eggs in a single year, the materials of which may enable 200 chicks to complete 98 per cent. of their growth at hatching.

C. F. HODGE

CLARK UNIVERSITY

The Development of the Chick: An Introduction to Embryology. By FRANK R. LILLIE. Pp. xi + 472. 251 Figs. New York, Henry Holt and Company. 1908.

The intention of the author of this book is to present in a simple, straightforward way the essential facts of the development of the chick for the use of beginners in embryology. This purpose has been to a very large degree realized, despite the ever-present temptation to enter into comparative discussions.

The book is divided into two sections, the first of which is devoted to a description of the formation of the embryo and is to an exceptional degree original and excellent. The second part, consisting of eight chapters on the development of the organ systems from the beginning of the fourth day of incubation to hatching, occupies a little more than half of the work.

The introduction is a statement of certain embryological theories and facts of general interest which can not properly be included in the body of the book.

The first chapter is a description of the structure, chemical composition and formation of the egg.

So little is known of the processes which occur while the hen's egg is in the oviduct that a consistent account of them is impossible. Consequently, Dr. Lillie in the second chapter bases the description of the development before laying, which includes the fertilization, maturation and cleavage of the ovum, and the formation of the ectoderm and entoderm, upon the work of Harper, Patterson and Blount upon the egg of the pigeon. This is an unavoidable exception to the author's rule of limiting the description to the development of the chick.

The third chapter contains a variety of material such as an outline of development, a statement of the orientation of the embryo in the egg, a discussion of the methods of classifying embryos, and an excellent table of the time of appearance and rate of differentiation of the organs. It would be wiser, perhaps, to omit this chapter because it makes too great a break in the account of the development of the embryo and contains much that the student can not yet understand. The content of the chapter could well be used elsewhere: for example, the section on orientation should be used, I believe, in connection with the account of the formation of the entoderm, given in the preceding chapter. The table, being merely for reference, should be used as an appendix.

The fourth chapter, entitled, From Laying to the Formation of the First Somite, is divided into four sections. The first is a description of the blastoderm in the unincubated egg and amounts to a review of the latter part of the second chapter. The primitive streak is described in the second section under four heads: Whole Views, Sections, The Head Process and Interpretation of the Primitive Streak. This division, especially the separation of the description of the whole views from that of the sections of the primitive streak, seems unwise. Four views of the blastoderm and three sections of the primitive streak of the sparrow, copied from Schauinsland, serve only to show by contrast the excel-

lence and fidelity of Dr. Lillie's figures. The last two sections of the chapter, describing the mesoblast of the opaque area and the germ wall, are very satisfactory. The chapter as a whole is a good account of the origin of the mesoderm.

The fifth chapter, Head-fold to Twelve Somites, is divided into parts upon the head-fold, fore-gut, neural tube, mesoblast and an embryo with ten somites. The attempt to describe the growing embryo, as a whole, breaks down in this chapter, and it might have been better to begin the part upon organogeny at the end of this chapter or earlier instead of after the next chapter. As it stands, portions of this chapter and of the next anticipate statements which are made later.

The second part of the book contains, in addition to chapters on each organ-system, a chapter on the external form of the embryonic membranes and one on the body cavities, mesenteries and septum transversum. There is an extensive and useful bibliography.

The book contains the inevitable errors of a first edition. There is no mention of the origin of the feathers, of the lymphatic vessels and of the muscles of the eye. The anterior division of the embryonic heart is called the bulbus arteriosus, or the bulbus, in the text, index and the original figures, but is named the bulbus cordis in copied figures. The choroid fissure is said to provide "an aperture in the wall of the optic cup for the entrance of the arteria centralis retinæ" (p. 166), but the author himself says elsewhere "There is no arteria centralis retinæ in the bird's eye" (p. 281). Happily such slips are infrequent.

The book contains a large amount of new material, for in addition to the second and fourth chapters upon the development of the egg before laying and upon the origin of the mesoderm, which embody difficult and fundamental research, it makes many small contributions to our knowledge of the embryology of the chick. The value of the book is greatly enhanced by this original matter, which, although it usually serves only to decide between conflicting opinions or to add small details, and although in conformity with the purpose of the book discussion of the literature and of interpretation is reduced to a minimum, gives a great store of facts that will be constantly referred to by students of embryology.

The numerous figures are well chosen and executed, but the publishers have poorly reproduced a number of them. More than half of the figures are new and among these are some very excellent drawings of whole embryos, and new diagrams of the structure of the egg and of the embryonic membranes. Some of the figures of sections could well be replaced by drawings of models.

The typography of the book is unusually good.

Professor Lillie's book, being a comprehensive and accurate statement of the processes by which the body and organs of a single animal are formed, will be of great service in the class-room where the careful observation and correlation of phenomena giving a training in true scientific method, are of more value than a broad and vague knowledge of many things and theories.

Leonard W. Williams

A Canyon Voyage. By Frederick S. Dellenbaugh. New York, Putnams. 1908. 50 plates.

Major Powell's "Exploration of the Colorado River of the West" is famous as a daring enterprise of forty years ago. His first river voyage in 1869 was briefly chronicled in his official report of 1875, and described in a more general manner in a popular book of later issue. His second voyage through the canyon in 1871 from the same starting point at Green River, has never been adequately described, although the results of observations then made were incorporated in the report above cited. At this late date, Dellenbaugh, a member of the second party, who has already written the "Romance of the Colorado River," in which earlier explorations are described, now gives us what he regards as a sequel to his previous book, in the form of a minute narrative of the second boat trip down the river, when he was artist and assistant topographer of the party. There is no attempt at scientific discussion, but a faithful effort is made to record every

item of the journey. In these later years, when amateur travel in the west is frequent, a detailed record of this kind will be of value to seekers after adventure; even if certain parts of the river are unduly dangerous, there are other long stretches in which a boat trip might well be undertaken in a summer vacation without too great disregard of a safe return home. Whether made by scientist, hunter or artist, the journey would surely be repaying in high degree, as one may be assured from the plates, as well as from Dellenbaugh's vivid descriptions. The solitude must be impressive as one floats down the smooth reaches beneath a mighty architecture of bare cliffs. The excitement of running rapids would seem to be sufficient for the most ardent seeker of new impressions. Many of the plates are excellent, although reproduced from photographs taken nearly forty years ago.

W. M. D.

SPECIAL ARTICLES

THE POSSIBLE ANCESTORS OF THE HORSES LIVING UNDER DOMESTICATION[1]

DURING the later part of the nineteenth century, it was generally taken for granted (1) that "the seven or eight species of Equidæ now existing are all descended from an ancestor of a dun colour more or less striped";[2] (2) that the common ancestor of the living horses, asses and zebras was connected by a single line of descent with the four-toed "fossil" horses of the Eocene period; (3) that the domestic horses are descended from Pleistocene species characterized by large molars with a long anterior internal pillar, a large heavy head and coarse limbs; (4) that in various parts of Europe and Asia, domestic races increased in size and were improved in make, speed and disposition, as a result of artificial selection and favorable surroundings.

On the continent it seems to be still generally assumed that the domestic breeds have descended from a single species,[3] but in England and America many naturalists now believe: (1) That domestic horses have sprung from several wild species connected by several lines of descent with the three-hoofed "fossil horses" of the Miocene period, and (2) that while some of the wild ancestors were adapted for living in the vicinity of forests and upland valleys, others were adapted for a steppe, plateau or desert life.

Of possible ancestors of the domestic breeds, the following may be mentioned: *Equus sivalensis, E. stenonis, E. gracilis* (Owen's *Asinus fossilis*), *E. namadicus*,[4] *E. fossilis* and *E. robustus*.

These species mainly differ in the teeth, size and deflection of the face and in the bones of the limbs. In the first three species the grinding surface of the anterior internal pillar (a fold of enamel on the inner surface of the cheek teeth) of the premolars and first molar, is short—in the last premolar, pm. 4, it may only be one third the length of the crown—in the second three species the anterior internal pillar of pm. 4 and m. 1, is long—at least half the antero-posterior length of the crown. One of the ancestral types (*E. robustus*) was broad-browed and had a short face almost in a line with the cranium; another (*E. sivalensis*), also broad-browed, had a long tapering, strongly deflected face; a third (*E. fossilis*) had a long narrow face, not so strongly bent downwards as in *E. sivalensis,* and a fourth (*E. gracilis*) had a fine narrow, but only slightly deflected, face.

In *E. gracilis* the middle metacarpal (cannon bone) was so slender that the length was seven and a half times the width, while in *E. robustus* the length of the metacarpal was sometimes only five and a half times the width.

Of these possible ancestors, the first three occur in Pliocene deposits, the second three have only hitherto been found in Pleistocene deposits.

[1] Abstract of a paper presented to the Royal Society, London.

[2] Darwin, "Animals and Plants," Vol. II., p. 17.

[3] The latest suggestion is that domestic horses are the descendants of *Equus fossilis* Rütimeyer, a Pleistocene species closely allied to the wild horse of Mongolia—*E. przewalskii*.

[4] *E. namadicus* seems to be closely allied to *E. complicatus*, a species widely distributed in North America during the Pleistocene period.

Equus sivalensis of the Siwalik deposits of northern India is the oldest true horse known to science (*i. e.*, the oldest one-hoofed horse with long (hypsodont) molars), and as it measured about fifteen hands, it is the largest of the old world "fossil" horses. This ancient Siwalik horse was characterized by long, fairly slender limbs and a long tapering face deflected to form an angle of nearly 20° with the base of the cranium. In addition to having a large head, a convex profile and long limbs, *E. sivalensis* seems to have been characterized by a long neck, high withers and a tail set on so high that the root was well in front of the point of the buttock.

Nothing is known of the ancestors of the horse which suddenly made its appearance in Pliocene times amongst the foothills of the Himalayas, but it may be safely assumed that it very decidedly differed from *Pliohippus*, the small "fossil" horse of the late Miocene and early Pliocene deposits of America from which some believe all the recent Equidæ are descended.

It used to be said that *E. sivalensis* could not be regarded as an ancestor of domestic horses because of the shortness of the anterior pillar of the cheek teeth. I find, however, that in some modern horses, the anterior pillars are decidedly shorter than in *E. sivalensis*, and that in some of the short-pillared domestic horses the face is nearly as strongly deflected on the cranium as in *E. sivalensis*. There is hence no longer any reason for assuming that this ancient Indian species had no share in the making of domestic breeds. But in the absence of a large and representative collection of skulls of domestic horses, it is impossible to say which modern breeds are most indebted to the large-headed, long-limbed race, which in Pliocene times frequented the area to the east of the Jhelum River, now occupied by the Siwalik Hills.

Mr. Lydekker thinks *E. sivalensis* or some closely allied race "may have been the ancestral stock from which Barbs, Arabs and Thoroughbreds are derived." When more skulls are available for study and when the phases through which equine skulls pass during development and growth have been worked out, it will probably be ascertained that broad-browed horses with a prominent interorbital region—a forehead convex from side to side as well as from above downwards—and a long tapering strongly deflected face have in great part descended from a species closely allied to *E. sivalensis*, but that slender-limbed horses with a broad flat forehead, and the face short and nearly in a line with the cranium, are at the most only remotely related to *E. sivalensis*.

Further enquiries will probably also show that some Indian breeds, as well as some of the unimproved races of Central Asia (*e. g.*, certain long-faced Kirghiz horses with a sloping forehead and long ears) in many of the points agree with *E. sivalensis* of the Pliocene deposits of northern India.

The second possible ancestor mentioned is *Equus stenonis* of the Pliocene deposits of Europe and North Africa. In a typical specimen of this species with the teeth in an intermediate state of wear, all the anterior pillars of the premolars and molars are shorter than in *E. sivalensis*, while in a specimen with the teeth well worn, the longest pillar may be only one third the length of the grinding surface of the crown—at no age are the pillars of the molars more than half the length of the crown. Whether the face was long and tapering and strongly deflected in *E. stenonis* has not yet been determined, but from the limb bones collected, it is evident that the horse with short-pillared molars, which in Pliocene times frequented the valley of the Arno, sometimes reached a height of nearly fifteen hands.

It is generally supposed *E. stenonis* either became extinct towards the close of the Pliocene age or was modified to form varieties with long-pillared molars. It is conceivable that some of the descendants of *E. stenonis* acquired long-pillared molars, but it by no means follows that all the Pleistocene horses of Europe with the anterior pillars more than half the length of the crown are related to or derived from *E. stenonis*—some of them may have been the descendants of *E. namadicus*.

Be this as it may, horses with teeth of the *E. stenonis* type existed in the south of Scotland during the first and second centuries, and horses with short-pillared cheek teeth are still in existence. In some of the skulls from the Roman fort at Newstead, the anterior pillar of the third and fourth premolars only measures 9 mm.—is only about half the length of the pillar in *E. namadicus* and other "fossil" Pleistocene species. Further, in one of the first century Newstead skulls, the first premolar is as large as in *E. stenonis,* and the face (as broad and long as in *E. sivalensis*) forms an angle of 18° 6′ with the cranium.

Further enquiries may show that the short-pillared species (with metacarpals as long but somewhat thicker than in *E. sivalensis*) widely distributed over Europe and north Africa, in Pliocene times played an important part in the making of Shires and other heavy modern breeds.

The only other possible ancestor dealt with in this contribution is the one to which I have given the name *Equus gracilis.*

Owen arrived at the conclusion that Pleistocene horses "had a larger head than the domesticated races" and that even in small varieties the teeth were nearly as large as in a modern cart horse.

Having come to these conclusions it is not surprising that when it fell to his lot to describe small equine molars from the drift overlying the London Clay and from a cavernous fissure at Oroston, near Plymouth, he decided that they could not belong to a true horse and (on the assumption that they belonged to an extinct ass or zebra) formed for them the species *Asinus fossilis.* In addition to the small second and third molars described and figured by Owen, there is in the British Museum a small first molar from Oreston. The anterior pillars of the second and third Oreston molars are more than half the length of the crown, as in horses of "forest" type, but the pillar of the first molar, m. 1, from Oreston is only about one third the length of the crown, as in *Pliohippus* and *E. stenonis.* Except in size, the small teeth from Oreston and other Pleistocene deposits bear little resemblance to the molars of asses or zebras, but they are practically identical in enamel foldings, as well as in size, with the molars of a small (12.2 hands) slender-limbed horse in the possession of the Auxiliaries who garrisoned the Roman fort at Newstead in the south of Scotland about the end of the first century.

In addition to small equine teeth the Devonshire Pleistocene deposits have yielded a small slender metacarpal. This metacarpal (from Kent's Cave near Torquay) is 220 mm. long and 30.25 mm. wide—the length is hence 7.27 times the width, as in fine-boned Arabs.

As might have been anticipated from a study of the teeth, the Kent's Cave metacarpal belongs to a very much finer-limbed race, than the small horse of the "elephant" bed at Brighton. On the other hand, the Kent's Cave metacarpal very closely agrees with the metacarpals of the small Newstead horse. This small first century horse in teeth and limbs agrees with Exmoor, Hebridean and other ponies of the "Celtic" type, *i. e.,* with ponies characterized by a small fine head, large eyes, slender limbs, five lumbar vertebræ and by the absence of the hind chestnuts and all four ergots.

It hence follows that the small equine of the English Pleistocene (Owen's *Asinus fossilis*) instead of being an ass or a zebra, is a true horse which in the metacarpals as in the "pillars" of the premolars and first molar, differs but little from *Pliohippus* of the late Miocene and early Pliocene American deposits.

Remains of a small horse with teeth and limbs like *Equus gracilis* (*Asinus fossilis* Owen) have been found in the Pliocene deposits of Italy and France, and in the Pleistocene deposits of France and north Africa. The Italian and Auvergne slender-limbed horse has generally been regarded as a small variety of *E. stenonis.* By Pomel and other paleontologists this French variety was known as *E. ligeris,* while the north African variety, named *Equus asinus atlanticus* by Thomas, was regarded by M. Boule as closely allied to, if not the ancestor of zebras of the Burchell type.

The slender metacarpals from the valley of the Arno and Auvergne so closely resemble the Kent's Cave metacarpal and the teeth from Perrier and Puy de Dôme in France and Lake Karar in Algiers so closely resemble the small teeth from Oreston, that *E. ligeris* and *E. asinus atlanticus* may be regarded as varieties or races of *E. gracilis*.

There are good reasons for believing that *E. gracilis* varied to form a northern and a southern variety. Remains of a slender-limbed northern race have been found in deposits belonging to the Neolithic, Bronze and still later ages in Britain and on the continent. At the present day the purest representative of this northern variety is the "Celtic" pony. Hence this northern variety may be known as *Equus gracilis celticus*.

Remains of a slender-limbed southern variety have not yet been found in recent deposits in north Africa, but fine-limbed ponies without ergots and hind chestnuts are sometimes met with in the south of France, and slender-limbed horses without hind chestnuts —horses almost certainly of north African descent—are occasionally met with in the West Indies and Mexico. In the French and still more in the wartless ponies of Mexico the limbs are longer than in the Celtic ponies, the coat is finer, the mane less full and the "tail-lock," so well developed in the northern variety, is very small. As the southern variety in all essential points agrees with Professor Ridgeway's fine bay horse of north Africa (*E. caballus libycus*) it may be known as *E. gracilis libycus*.

Slender limbs and the absence of ergots and hind chestnuts, are apparently as distinctive of members of *E. gracilis* as an upright mane and the absence of hind chestnuts are distinctive of asses and zebras. Hence when as a result of crossing slender-limbed individuals without ergots and hind chestnuts appear in any area it may be assumed that the horses of that area include *E. gracilis* amongst their ancestors.

From enquiries made and from crossing experiments, it has been ascertained that ponies of the "Celtic" type occur in the Färöe Islands and Iceland, in the western islands and highlands of Scotland, in the west of Ireland, in Wales, Exmoor and the New Forest and in Norway and Finland.

Further crossing experiments have made it evident that the yellow-dun fjord horses of Norway are mainly a blend of the "Celtic" and "forest" types, that the Shetland ponies, though usually having the conformation of the "forest" or *E. robustus* type, are in part of Celtic origin, and that some of the mouse-dun Tarpans of the Russian steppes are a nearly equal blend of the Celtic and *E. przewalkii* or "steppe" types.

Professor Ridgeway arrived at the conclusion that in the fine bay horse of North Africa there is a frequent tendency to stripes on the back, legs, shoulders and face, to a blaze on the forehead and to white "bracelets." Experiments made with four types of Arabs and with Russian, Mongolian, Indian and Borneo ponies, English, Irish, Iceland and Norse ponies, support the view that the Pleistocene ancestors of the modern slender-limbed ponies with short-pillared molars was of a yellow or bay-dun color with a narrow dorsal band and bars on the legs, but had neither "bracelets" nor a blaze.

As stripes are most numerous on broad-browed horses with coarse limbs, they have probably in most cases been inherited from ancestors of the "forest" or *E. robustus* type.

As to the part played by *E. gracilis libycus* in forming domestic breeds, nothing very definite has been made out. Professor Ridgeway says all the improved breeds of the world are a blend in varying degrees of the bay horse of North Africa with thick-set, slow, dun and white horses of Europe and Asia allied to *E. przewalskii*. A number of hybrids bred at Woburn by the Duke of Bedford afford little, if any, evidence in support of the view that Barbs, Arabs or thoroughbreds include amongs their ancestors horses of the Prejvalsky or "steppe" type. Slender-limbed horses with a wide flat forehead and a nearly straight profile appear to be a blend of *E. gracilis libycus* (Ridgeway's *E. caballus libycus*) and horses of the *E. robustus* ("forest")

type, while slender-limbed strains with a fine narrow face, a well set-on tail and a mane that clings to the neck, probably most accurately reproduce the variety of *E. gracilis* which in prehistoric times inhabited north Africa.

J. C. EWART

UNIVERSITY OF EDINBURGH

THE SOCIETY OF AMERICAN BACTERIOLOGISTS[1]

The Etiology of Plant Tumors: ERWIN F. SMITH. (Illustrated by numerous stereopticon photographs.)

The author prefaced his remarks with the statement that Dr. C. O. Townsend and Miss Nellie Brown were associated with him in the prosecution of this research. The address consisted of a series of lantern slides with running comment. The slides showed inoculated plants of various species and of widely different families, the growths in all cases being the result of pure culture inoculations of the schizomycete *Bacterium tumefaciens*. The crown gall of cultivated plants is cross inoculable to an astonishing degree. In susceptible tissues the signs of these galls appear within as short a time as four or five days. The tumor continues to grow for several months and in some cases for several years and may become 5 cm. or more in diameter. Hundreds of pure culture inoculations have been made. The organism cultivated from the Paris daisy has been inoculated many times successfully into the same and also into the peach, rose, hop, sugar-beet, white poplar and other susceptible plants. That from the crown gall of the peach has been many times successfully inoculated into the peach, and also into the Paris daisy, sugar-beet, hop and other plants. The schizomycete from the hop has been inoculated successfully into the hop and into the Paris daisy, sugar-beet and other plants. One of the astonishing things about this crown-gall organism is the number of families which are subject to infection; in other words, the very simple and generalized nutritional needs of the parasite. In some ways it resembles the root-tubercle organism of Leguminosæ, but is not identical. It has been inoculated into clovers with the production of knots. Quite recently from the hard gall of the apple (selected by Dr. Hedgcock) we have isolated an organism which appears to be like that occurring in other crown galls, and with this, hard galls have been produced upon the Paris daisy. A similar if not identical organism has also been isolated from the hairy root of the apple and successfully inoculated into the sugar-beet, *i. e.*, with the production of similar root-tufts at the point of inoculation. There is now little doubt, therefore, that the hairy root of the apple is also of bacterial origin.

Metastatic growths occur on these plants, but up to this time we have not definitely determined the channels of infection from the primary tumor to the secondary ones. These are easily discovered in the case of the olive-tubercle, but are not readily found in case of these crown galls. The same remark is true respecting the bacteria in the primary tumors. They are very abundant and easily discovered in the olive-tubercle, but not readily detected in the crown gall, although obtainable therefrom in Petri dish cultures on agar. It is still too early in the course of our studies to make positive statements respecting the likeness or unlikeness of these growths to malignant animal tumors, but it is proposed to continue this phase of the inquiry. There is in these growths a very rapid multiplication of parenchymatic tissues with reduction and distortion of the firm conductive tissues of the plant and the final decay and sloughing off of the spongy tissues, leaving open wounds, on the margins of which fresh developments of the tumor may appear.

Seed Corn as a Means of Disseminating Bacterium Stewarti: ERWIN F. SMITH.

In the summer of 1908, in a hothouse on the grounds of the Department of Agriculture, the writer succeeded in obtaining from plants grown from a suspicious seed corn eight times as many cases of Stewart's disease as from plants grown from disinfected seed taken from the same sack. The plants were treated in all respects exactly alike, except that a portion of the seed corn was planted without disinfection and other portions were subjected for ten minutes and fifteen minutes to 1:1,000 mercuric chloride water. The mere statement that there were eight times as many cases in the plants grown from untreated seed by no means expresses the whole truth, because in the plants grown from treated seed all the cases, with four exceptions, were very slight ones, every plant being reckoned as diseased which showed a

[1] In session December 29–31, 1908. These abstracts were received after the general report of the society had been printed in the issue of SCIENCE for June 25.

trace of the yellow slime in the bundles of the stem, although most of the plants so included contained very few affected bundles and did not yet show distinct secondary signs. In the plants grown from untreated seed there were many severe cases of the disease, the entire foliage being dried out and the bacterial multiplication in the vessels of the stem being several thousand times as much as in the cases which appeared in the treated plants. The seed corn was considered as suspicious for two reasons: (1) earlier in the season some plantings in Virginia made from this particular sample yielded many diseased plants, and (2) the corn was grown by a man to whose farm the writer traced a similar outbreak of this disease some years ago.

Various yellow bacteria were obtained from the surface of this corn by means of agar poured-plates, but not *Bacterium Stewarti*, although many attempts were made to isolate it. The latter consequently could not have been very abundant, at least in a living condition, and this may in part account for the small number of cases obtained (10 per cent.). The total cases on the untreated plots were 185 out of 2,017 plants; on the treated there were 36 cases out of 2,370 plants, 32 of these cases being only slightly diseased.

The Occurrence of Bacterium pruni in Peach Foliage: ERWIN F. SMITH.

Some years ago the speaker made inoculations (by spraying) on the foliage of some peach trees standing in his back yard. The schizomycete used was derived from the black spot of the plum. The sprayings were made late in July, at sunset in rather dry weather. The trees were not covered by tents and the spraying was not continued through the night, as it should have been. The next morning the foliage was dry. After some days, a few bacterial leaf spots developed on one of these trees—perhaps fifty altogether, and a microscopic examination demonstrated the presence of pockets of bacteria in some of these spots, but the experiment was considered in the light of a failure because very few spots appeared in proportion to the amount of culture fluid used, and none at all on one of the trees. Subsequent studies on plums led to the belief that the reason for this failure was due to the age of the foliage and to the fact that the moisture did not remain sufficiently long to secure many infections. In the summer of 1907 these experiments were repeated in one of the hothouses of the Department of Agriculture under conditions which could be more readily controlled. Healthy seedlings and grafted plants of two ages were placed close together on a bench in one corner of one of the hothouses, and heavy canvas was hung in front of the two exposed sides of the bench, so as to make an enclosed area which could be kept moist. River water was then syringed upon the plants (the same that the plants had been in the habit of receiving) and afterwards water dilutions of pure cultures of *Bacterium pruni*, obtained from Arkansas plums. The tent cloth was kept on for two days and the sprayings were repeated at intervals, so that with the exception of two short periods during which the foliage became dry by accident, the moisture was retained upon the leaves throughout the time that the tent cloth was employed. The result of this experiment was the appearance, after a number of days, of several thousands of typical leaf spots. A microscopic examination of many of these spots demonstrated a great number of bacteria in the center of the same, and poured-plate cultures were made therefrom. To obtain numerous spots it is necessary to make inoculations early in the growing season, that is, in May, and to keep the foliage moist from twenty-four to forty-eight hours. The spots obtained on the peach leaves differed in no way from those occurring naturally on peach, and there can be no doubt that the leaf spot of peach is identical with that of the black spot of the plum, both being due to *Bacterium pruni*. All of my experiments, however, have been made with the organism taken from the plum.

Two Sources of Error in the Determination of Gas-production by Microorganisms: ERWIN F. SMITH.

1. Some microorganisms produce gas from inosit (muscle sugar), consequently in bouillon agar shake-cultures, to which various sugars have been added, the resulting gas bubbles can not be attributed to the sugar added until it is known (*a*) that inosit does not occur or (*b*) that the organism is incapable of fermenting inosit.

2. Some gas-forming microorganisms, *e. g.*, a yeast recently isolated by the writer, do not liberate gas unless the cultures are shaken or stirred, *e. g.*, with a platinum needle. Then gas is evolved abundantly. Ignorance of this fact might sometimes lead to error. It is a fact well known in the Caucasus that the kefir ferment takes place most satisfactorily if the leathern sacks are frequently shaken, and it is said to be the habit for each member of the family to give the sack a kick in passing by it.